MENTAL LIFE OF TWO *MACACUS RHESUS* MONKEYS IN CAPTIVITY.—I.

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INTRODUCTION.

To Dr. Edward L. Thorndike, of Columbia, belongs the honor of having first taken the monkey into the psychological laboratory. He experimented for several months with three *Cebus* monkeys from South America. His methods and work have been made a starting point for the following studies. In my experiments two *Macacus rhesus* monkeys from India have been used. This species is peculiar in that it is a connecting link between monkeys and baboons. It is a favorite among trainers, and is regarded as very intelligent, though not so intelligent as the *Cebus*. No extensive studies have yet been made of it from the standpoint of psychology. Cuvier, however, gives a charming story of a mother *Macacus rhesus* and her young. Not having access to the original report, I have translated it from Brehm, and the translation will be found in the appendix to this paper. So far as I know his is the only careful study yet made of any of the psychical qualities of the *rhesus*, though there are extant many scraps of information concerning them of more or less scientific value.

I wish here to thank President G. Stanley Hall for suggesting the investigation and for valuable assistance in procuring the literature of the subject. I owe acknowledgment to Dr. E. C. Sanford, likewise, for numerous suggestions, and assistance in procuring and arranging of apparatus, and for help and criticism both in the experiments and in the evaluation of them. I wish also to thank Mr. Wilson, the Librarian of the University, for assistance rendered in the library, Dr. Hodge for the use of his fine camera, and many others for valuable assistance given.

METHODS OF COMPARATIVE PSYCHOLOGY.

METHODS OF SECURING DATA.

Kline regarded the methods of studying animal psychology as of two sorts: (1) the natural method, consisting in careful and continuous observations of the free life of an animal. In this group belong Huber, Moggridge and McCook on ants,

Audubon on birds, Figuier on insects, and Mill on our domestic animals; and (2) the experimental method, which consists in putting a question, and subjecting the animal to such conditions as will favor the performance of activities that shall contribute material for answering the question asked. While the classification is a correct one I desire to divide still further, and to indicate five methods of studying the characteristics and capacities of animals. Each has its advocates, and valuable results have been obtained by all of them. Some render only qualitative data, others both qualitative and quantitative.

(1) *Free observation of animals in their native habitat.* Disciples of this plan are to be found in Ernest Seton Thompson, W. J. Long, Paul du Chaillu, John Burroughs, Lubbock, Audubon and Forel. The first four of these are literary. While they attempt careful observation of the animals, they do not hesitate to read into them all sorts of feelings and mental capacities, surpassed only by the fertile imagination of Uncle Remus. The method from a literary standpoint fully commends itself, but such studies must not be mistaken for science. If the method is to add anything to the stock of human knowledge the literary observer must take the greatest care to keep the foundation of his work entirely within the field of truth. But adding to the sum of human knowledge is not the only outcome of study worthy of attainment. The literary observers are adding a rich fund to the sum of human sympathy and interest, and that is enough so long as in making that addition fundamentally false conceptions of the real animal life are not inculcated.

But from a purely scientific standpoint also, this method can be made to bear first-class fruit. It has the advantage of seeing the real animal in his natural, unhampered reactions. Lubbock and Forel studied the smaller animals and insects in this way. It is an easy matter to bring whole colonies of these under direct observation and yet to leave them much of their native freedom. The larger animals cannot well be studied by this method. It can hardly, save for the lowest forms and smaller animals, ever be more than qualitative in its results, though Lubbock has demonstrated that for ants, bees and wasps the method can be made to yield both qualitative and quantitative results.

(2) *The second method* is ably presented by Wesley Mills in his *Animal Intelligence*, p. 7. A single quotation from him with reference to the study of a dog will give a fair idea of the method which he proposes and uses.

"Not only is it necessary in order to understand the individual dog to begin with him at his birth and to follow his history throughout, but such a course is essential for the com-

prehension of the nature of dogs in general, and, personally, I am deeply convinced of the importance of such investigations, after having been engaged in them for some years." In other words the young animal is to be studied rather than the adult, but the study is to comprehend his gradual development as well as his reactions in his natural environment. "When such studies are carried out on representatives of different groups of animals, and on different breeds or individuals, one's conception of the true nature of animal intelligence—or, to use a more comprehensive term, the psychic life of animals—is vastly widened and altogether more correct in every respect."

The method is invaluable in a study of instincts, individual differences, growth, development, and the influences of environment. No other methods can hope for results more significant for genetic psychology and education. The stages of development can here be seen to pass along from germ to maturity in very rapid succession. Yet attempts to carry its results over into the realm of human activities are always to be guarded with great care. If students of animals are always in danger of reading their own processes, unduly, into the animals that they study, those that apply the results of such studies to human conditions are similarly liable to read animal conditions into the psychic life of man. It will be recalled that Comenius, probably more than any one else, in this way, made nature the criterion of methods in a somewhat characteristic pedagogical scheme.

However valuable this method, it has its limitations. While it brings out what the young animal *does* it omits both what it and the adult *can do*. Only a limited variety of animals can be so studied at all, and a still more limited number, while reacting unhampered by human hand in their relation to their natural parents. The method has been successfully applied in studies of chicks, kittens, puppies and squirrels.

(3) *Training*. This method is presented by P. Hachet-Souplet in his *Examen Psychologique des Animaux*. He proposes to study the animal by training it, holding that rational training multiplies for the animals, especially for the superior animals, that is to say the most interesting ones, the occasions for bringing into play, before the observer, their intellectual and instinctive faculties, enabling him not only to arrive at significant conclusions but easily to verify them. The animal is to be brought by repetition to such a stage that he shall know by a verbal order or by a gesture from the trainer just what he is to do. Thus we have the animal studied in action. The method reveals but little beyond the powers of memory and association. Hachet-Souplet proposes that animals be classified on the basis of psychic capacities and that suscepti-

bility to training be taken as the proper criterion for determining mental capabilities. On this basis he makes three classes of animals. (1) Those capable of being persuaded to perform their parts. Such are said to act chiefly through intelligence. (2) Those which cannot be persuaded but must be coerced. These are said to act through instinct. (3) Those in which excitation only is possible. They possess, evidently, neither intelligence nor instinct, they are simply excitable and live without psychic direction, only by repetition of physical and chemical phenomena, which determine and continue their nutrition and reproduction. If memory and association were the only signs of intelligence this scheme for the classification of the psychic faculties would be complete though very general. To say the least the scheme is ingenious and deserves serious consideration.

(4) *Free observation of animals in captivity.* This method was employed by Cuvier, Romanes and Garner in their studies of monkeys. Probably no other method suffers so great limitations. The caged animal often ceases to be himself. He varies from insipid tameness and moroseness to wild excitement, and is in several ways very different from what he is in his larger freedom. The method is in reality but an application of the first method described above to a single case of the one yet to be described. It is a free observation of an animal's reaction to a single kind of apparatus—the cage. Those employing the method, for the most part, have observed loosely and have interpreted rather liberally. The method is only semi-scientific, and the results that have been obtained have not always been very reliable. Such expressions as "A great many times," "A great many monkeys," etc., are too loose for scientific use. Studies on this plan have ascribed to the animals observed all sorts of capacities. In addition to loose observation, Mr. Romanes has unfortunately given credence to many stories by hunters and other untrained persons where the details of the stories were wholly omitted. Very often when one has the full situation the wonderful achievements of the animal drop to the level of the common-place. The reader will find such an illustration in my report of the monkey's reaction to the trees, given in the next section. Besides the fact of imperfect observation the ordinary observer is prompted by an inordinate desire to exalt the powers of his "pet dog Solomon," and make him appear as wonderful as possible. But when put to careful test the wisdom of "Solomon" is often found to fall below the level of that of the ordinary cur. Among the students who have ascribed undue value to these current stories of animal intelligence, the most prominent are Houzeau, Lind-

say, Romanes and Weir. The great Darwin himself has not wholly escaped this tendency.

We would commend this method as a partial one, but like other methods, it needs the checks and control tests of other methods to keep it in the straight and narrow way. I count it a poor method but one by no means to be wholly abandoned.

(5) *Experimental.* This method consists essentially in subjecting the animal to fixed or controllable conditions, and in noting its qualitative and quantitative reactions. Lloyd Morgan, Kline, Small, Thorndike, and others, as is well known, have employed this method somewhat extensively. By way of example, it may be well to present here Thorndike's explanation of the method, when dogs and cats were the subjects.

"By this method of experimentation the animals are put in situations which call into activity their mental functions and permit them to be carefully observed. One may, by following it, observe personally more intelligent acts than are included in any anecdotal collection. And this actual vision of animals in the act of using their minds is far more fruitful than any amount of histories of what animals have done without the history of how they did it. But besides offering this opportunity for purposeful and systematic observation, our method is valuable because it frees the animal from any influence of the observer. The animal's behavior is independent of any factors save its own hunger, the mechanism of the box it is in, the food outside, and such general matters as fatigue, indisposition, etc. Therefore the work done by one investigator may be repeated and verified or modified by another. No personal factor is present save in the observation and interpretation. Again, our method gives some very important results which are quite uninfluenced by any personal factor in any way. The curves showing the progress of the formation of associations, which are obtained from the records of the times taken by the animals in successive trials, are facts which can be obtained by any observer who can tell time. They are absolute, and whatever can be deduced from them is sure. So also the question of whether an animal does or does not form a certain association requires for an answer no higher qualification in the observer than a pair of eyes. The literature of animal psychology shows so uniformly and often so sadly the influence of the personal equation that any method which can partially eliminate it deserves a trial."

If the trials are continued for some time one has a large element of training, and the method comes to have a great deal in common with method four. With this method is afforded opportunity to study association, memory, imagina-

tion the senses, powers of generalization and reasoning, besides individual differences and educability. We get here, as in the method of training, not so much what the animal *does do* in his native environment as what he is *capable of doing* under somewhat artificial conditions. It alone can furnish opportunity for anything like exact quantitative work, while it possesses all the advantages of training so far as repetitions and critical observations are concerned. Its limitation is to be found chiefly in this, that it loses the native activities of the animals and does not use the signs ordinarily employed in training by persuasion without the stimulus of food.

One of the great difficulties in the employment of this method is that of selecting tests and apparatus properly related to the character of the animal studied. Taking only such activities as are wholly natural to the animal, and such apparatus as is easiest for him to operate, makes his reactions very simple, and the test fails to bring out his possibilities. On the other hand, the tests selected must not be too far removed from the field of his natural activity, for in such cases the animal is often completely baffled, and only the most limited results are obtainable. The animal may appear stupid because the test is unsuitable, and thus be easily misjudged.

The highest possible knowledge of an animal is hardly to be reached by the employment of any one of these methods. The methods are not equally applicable to all animals. Yet there is every reason for believing that if all are employed upon the same animal, as far as possible, we shall arrive at a more complete understanding of him. Not opposition to one method or undue emphasis upon another, but a reasonable use of each within the just limits of its applicability, should be the practice of the student of comparative psychology.

METHODS OF INTERPRETING DATA.

Wundt sets forth two points of view in animal psychology.[1] "We may set out from the notion of a kind of comparative physiology of mind, a universal history of the development of the mental life in the organic world. Then the observation of animals is the more important matter; man is only considered as one, though of course the highest, of the developmental stages to be examined. Or we may make human psychology the principal object of investigation. Then the expression of mental life in animals will be taken into account only so far as they throw light upon the evolution of consciousness in man." Wundt pursues the second of these courses, and insists that "not the least advance can be made, either in the psychology

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[1] Human and Animal Psychology, p. 340.

of a particular animal or in that of the animal kingdom without starting out from the facts of the human consciousness." In spite of all the warnings against reading our own powers and processes into the lower animals, when we undertake to study their psychic phenomena, I am not certain but that that very thing is necessary, within limits, in all fruitful studies of the mental life of animals. But the careful student of human and animal minds will avoid prematurely concluding that because he finds the mental factors and processes thus and so in one of these great fields of investigation, they necessarily obtain in the other in the same relation. The schematized evolutional life of animals possibly applies to man. But direct and unhesitating application should be questioned.

Again, the camp of comparative psychologists divides on the application of the *Lex parsimoniae* in the interpretation of their data. There can be no doubt but that the application of the law is a virtue and its rejection a vice. In practice, there are on the one hand extremists who ascribe to animals, with an almost pitiful blindness, and without limit, all sorts of human and angelic powers; on the other hand, there are other extremists who reduce every mental process of the animal to a common level in lowest terms—tropisms, instincts and associations. The former are virtually vicious and the latter are victims of their virtue. In the one camp we find steeped more or less in the vice, Romanes, Weir, Garner, and Mills; in the other, Wundt, Morgan, Thorndike and Loeb.

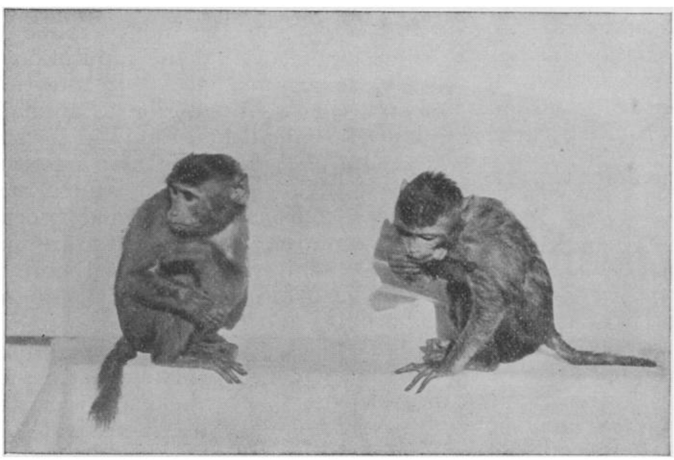
As in many other cases, the true practice is in all probability to be found between that of the extremists. My sympathies lie nearer to the "victims of the virtue;" nevertheless, it seems to me that we must not concede that the mental processes of the animal are so unlike human processes as to be absolutely simple.

If in any class of animals one can be shown to imitate, remember, invent or reason, then a complex reaction of any one of the class cannot safely be explained without reference to this possible process as a part of the complex. Over-simplicity is the rock of danger to the over-parsimonious.

General Observations.

My first monkey, a male *Macacus rhesus*, arrived Oct. 15. The female came about one month later. The male was said to be about eight months old, and the female, twelve. Jack is more nervous than his mate, Jill. His nervous mechanism is set off by little noises and threatenings which pass entirely unnoticed by Jill. Shipping had greatly frightened him. He sprang about the cage, threatening, and uttering a loud call by ones, twos, threes and fours. This call or scream is hard to

represent by letters or the human voice, but it is approached by *hwuich*. It appeared to be prompted by a feeling of loneliness, lostness, strangeness and hunger, and to be given as a call to learn the whereabouts of others of his kind. As a caged animal, his social instincts do not appear to be very highly developed, though in its natural habitat the species plays and goes foraging in bands. The calls were most vigorous on the first and second days and almost wholly disappeared in about a week. While experimenting he has given the call a few times with apparently no other stimulus than hunger. The frequency of these calls can be judged from two ten-minute records: Oct. 17, 21 times; Oct. 18, 18 times.



JILL. JACK.

When a live frog was thrown into the cage, he grabbed it but let it go immediately and sprang to his perch. He appeared to be taken by surprise, and was not really afraid of it, for he returned immediately, caught it up and pulled it to pieces using both hands and teeth in the process. He did not appear to do it as if destroying an enemy, but as a mere feelingless business matter, as if it were something that ought to be done and he did it. If he had any feeling about it, it did not betray itself. His face, like that of a baboon, for indeed he is very baboon-like, is almost totally expressionless. To me, it has as yet but two forms: one with the mouth closed and one with the mouth opened. The latter occurs when he crouches and threatens or tries to bluff one, and when he sees food and apparently thinks how good it would taste. I believe

that much of the monkey's expressions of triumph, villainy, jealousy, anger and risibility are either in the manner and speed of limb and body movement, or in the viscera of the observers who read into them a feeling like that which they suppose they would have under similar circumstances.

These monkeys show fright by hasty springs in the cage, or by running and looking back when loose in the large room. When baffled in an attempt to open a box for food Jack walked away uttering a mild audible sigh, *hweuwh*. Surprise is shown by a quick jerk of the body; anger, on the part of the male, at the collar, by quick jerky efforts to bite or tear it, and curiosity, by gentle peeps and lifting an object up with extended hand. He did this with a rattle box put into the cage; and, again with a paper sack containing a small snake, of which he was afraid, yet, curiosity led even to tearing a peep-hole in the bottom of the sack, through which, with much caution, he often looked at his hereditary enemy.

The gnawing, which was considerable at first and continues to some extent, appears to be prompted by the instinct of escape, and leads him to lay hold wherever there is good gnawing. Some of the gnawing was done about the door. I think it is safe to assume that if gnawing at any point had brought release, gnawing afterwards would have appeared to be much more purposive.

They have shown no signs of play in any way. They take life seriously. When I first saw Jack springing up and down on the pan in the bottom of the cage I was half inclined to take it as a play manifestation. But he afterwards did the same thing on the top of a box into which he was failing to make his way. It appeared to be a sort of useless effort at just doing anything to get into the box or get out of the cage.

When a newt, which looks something like a snake, was put into the cage the monkey showed no fear of it. He did not tear it to pieces, as he did the frog, but rolled it between his palms many times, and at last bit it through the head and threw it down.

Jack and a pigeon were equally frightened at each other when in the cage together. Yet at times he would swing down from the wire or perch as if to touch or grab it, but at the same time revealed his fear of it.

A full grown cat brought into the room showed no fear of the monkeys whatever, but they were desperately afraid of it. They produced the "*nqu-u-w*" sounds in profusion, sprang into the windows, and sought the farthest corners of the room, even engaging in a mutual spat for vantage places of safety. On the following day when they were turned out of their cages

they climbed into the windows and looked about everywhere for the cat.

I had been told that Dr. Thorndike kept his monkeys from touching things by putting a ball of cotton batting on them. Accordingly I covered a pear almost completely with cotton, pinning it fast with pieces of toothpicks and pulling the cotton out in a fluffy ball. I threw it into the cage. Showing no sign of fear whatever, he tore away the cotton with hands and teeth, and ate and pulled alternately until the pear was completely eaten.

On Oct. 20, I tied a rope to the end of Jack's little chain, and fastened the rope to a large post in the center of the room. After many accidents I succeeded in getting him properly tethered. He ran across his circular area of action receiving many jerks. But he profited by this and learned, except when scared, to avoid the jerking by carrying a foot of slack in his chain as he ran about or walked erect. He often paced thus along an arc of his circle until some twenty feet from his cage when he turned and ran back erect. But being frightened he broke the hook at the collar and gained his larger freedom. This freedom he has retained. When I waved a stick toward him he ran back to the cage. When the door of the cage is opened he usually goes in at once of his own accord. At a motion of my hand or at my calling "Get down, Jack," he gets down from a case or out of a window, especially if he sees that I am certain to approach if he does not do it. If I pretend to be at work and order him down in an ordinary tone of voice, without changing emphasis or inflection, he pays no attention. The monkeys have given no sign that they know their names.

The sounds produced by the male are the loud *hwuich* and the softer, *hweuwh*, an expulsion of breath through mouth and nostrils, a blowing of air out between the loosely held lips, and a grinding or a gritting of the teeth. These last seem to have no well defined purpose. All my efforts to reproduce these vocalizations have utterly failed to call out any response from them. The nostril and mouth expulsion I presume is the one represented by Mr. Garner by the formula, *nqu-u-w*. It is uttered most frequently when food is displayed and while eating, but may accompany disturbance by the keeper or by visitors. The sounds of the female are a loud call, corresponding to the male's *hwuich*, resembling the call of the woodpecker; a softer sound closely resembling the mew of a cat; the *nqu-u-w;* blowing between the loosely held lips; a baboon-like growl uttered when I kicked violently at her as if to strike her, and a high pitched screech.

In the early part of their larger freedom they spent much time sitting in the windows. They appeared interested in the

street cars and wagons that passed along the street at the other side of the campus and enjoyed the warm sunshine. Later, I cut and set up three small trees in the room. They showed no interest in the trees until they began to bear bits of fruit and bread. Here their instinctive skill and ingenuity in getting food from the most inaccessible tips of branches was of especial interest. They seemed to scan the limbs and plan long before reaching the limb where food had been placed, whether to bend the limb around to a stronger one or to follow it to the end and drop to the floor or swing back to the limb and pass down the trunk. When food was placed on a strong limb that extended several feet to one side and pressed against the ceiling, Jack climbed part way up, then backed out and went to an adjoining limb where he seemed to examine the situation in order to determine the best line of attack. After examining from two places he went up the limb to a part near the ceiling then swung under and went hand over hand until the food was reached. He grabbed the fruit, let go with his hands and crammed the food into his cheek pouches while hanging by his feet, head downward. This operation completed he returned to the floor. At one time food was put on the end of a stick which was fastened to a gas jet. The stick projected horizontally out toward the middle of the room. The food was so high that the monkey could not spring up to it. One of the trees stood near. He ascended a long slender limb fully five feet away from the apple. His weight bent the limb down to the apple and he won the prize. The over-zealous for reasoning in animals can here be sure that the monkey showed a comprehension of a mathematical principle and of the law of gravity. But such was not the case. He sat at the farther end of the room while the food was being arranged. From his position it would be difficult to tell just how far it was from the limb to the apple. Besides this the monkey had been fed often from the end of this limb. Seeing food arranged near there he merely followed the old track, and as he did so found himself gradually coming nearer and nearer to the goal. There was no reason about it, and certainly no comprehension of mathematical and physical principles.

After a few weeks the trees were used considerably as perches to which the monkeys often retreated between tests, especially if the rearranging of the apparatus required some time. The window perches have come to be used only occasionally.

When food was shown the male it will be remembered that he dropped his chin and opened his mouth. Under the same provocation the female worked her lips rapidly. Some interpret this as a sign of anger, bluff or assault. Mr. Garner thinks that it may be "intended as a vote of thanks." In my opinion the *rhesus* never gives thanks for anything. He is not of the

thankful sort. Nor yet does it express anger. It seems to me to be a kind of impatient nervous overflow intermingled with a notion of the taste and the eating activities to follow, while waiting for the food finally to be put where it can be reached, and is stimulated immediately by the sight of the food itself. The child represents the same physical excess by movements of the head, arms and legs, while he cries, "One for the money, two for the show, three to make ready and four to go." Jill has spent more time in the trees and less in the windows than Jack, shows less fright and is generally slower in all of her movements. No snakes, frogs or pigeons have been put into her cage.

The two have shown no spirit of play either separately or when together. A wheel like that used by squirrels was arranged, but it was never used except to secure food, and as a vantage point for seeing what was going on.

When out in the room, if food is thrown to them, both rush for it, but when either gets it the other respects the right of possession.

Sign language is used and understood somewhat. I have already spoken of the recognition of signs when delivered by the keeper. Twice the female struck with her hand when she thought the male was too near her food. If either is master it is she. When together, their favorite pastime is "flea hunting" which is really dust and dandruff hunting. Backing up in front of the mate, lying down or assuming a sort of pose is understood to mean "pick me." However, this sign language, if indeed it can in any sense be regarded as such, is only of the most rudimentary sort.

In general I may say that so far as I can learn, their reactions are in many ways like those of their wild state, and they show the same kind of ingenuity in meeting and solving their problems, in procuring food, in escaping from an enemy, and in finding their associates.

Repetition of Dr. Thorndike's Experiments.

Relation to Dr. Thorndike's Work.

Dr. Thorndike experimented with two *Cebus* monkeys from South America. He used a great variety of apparatus, consisting largely of designs with which food was associated, and of boxes having doors fastened with various kinds of latches. In these were placed food which the monkeys could get by moving the button or other device holding the door shut. I began my work by repeating, in the main, the experiments which he made. I have used in all thirteen kinds of fastenings for the doors of the boxes. Nine of these were afterwards put

into second positions on the doors. Besides this seven groups of these fastenings were used. Three string devices also and a windlass were used in connection with the cage. Opportunity was afforded for but few experiments on imitation, and none by putting the animal through a process to be learned, since this species of monkey, like most wild animals, objects to being touched or handled, unless one begins with them when they are very young. While they have crowded by me for food or pulled my hands open for it, I have yet for the first time to touch them actively. The memory tests have been made with apparatus not used by Dr. Thorndike, so I reserve a discussion of them for another part of the paper. Only a few pieces of my apparatus are identical with Dr. Thorndike's, but the general character of them is the same.

General Conditions.

Those who have seen monkeys in captivity and in their native habitats have noted in some of them great differences under the two conditions. The caged gorilla is sullen; the female chimpanzee is often morose and sits about in a drooping position or engages in swinging the body to and fro. I have seen the golden baboon, when not entertained, indulge in a swaying of the body which was a cross between a swing and a vertical movement. Most monkeys, if they age in captivity, become crabbed and treacherous, however affectionate when young. I am not aware just to what extent the *rhesus* shares in this change of character but it is certain that his ill disposition does not disappear.

It is safe to assume that all studies of animals in cages and pens would show variations from those of animals in their native haunts. Such studies with the *rhesus* are less unnatural than with South American monkeys, for example, since owing to the peculiar attitude of the natives of India toward monkeys, they have become bold enough to take up their abode quite in the seats of civilization. At home they are in a degree reverenced. They may be chased from fields, gardens and orchards, and may be shipped by steam car or caravan into distant regions, or trained and sold for a foreign market, but they must not be killed. These monkeys enter the villages in troops to beg and steal food at the stores. They are acquainted therefore with man and his ways and works, buildings, streets, etc. Hence, a rather free captivity such as this pair has had (they were loose in a very large room), does not subject them to a wholly new environment.

In experimenting, the monkeys were kept hungry enough to make them keen in their efforts to procure food. It can hardly be maintained that they were subjected by this to unusual

conditions, since in their wild state, it is no uncommon thing for them to become ravenously hungry. Starvation is always a menace and a not very remote possibility to all wild animals.

It has been urged that the tests to which monkeys as well as other animals are subjected by our fifth method, described in a previous section, are foreign to their apperceptive stock of ideas: that such experimenting is somewhat like asking a biologist or anthropologist to solve a difficult problem in higher mathematics, and by it the animals are put at a great disadvantage. They may appear to lack mental capacities, it is contended, simply because the tests employed are outside of their normal experiences and instincts. The criticism, so far as the Bhunder monkey is concerned, is only partly just. In his woody home he is engaged much of his time in searching for food. He must meet various situations in order to get fruit from the tips of slender limbs. Bark must be pulled away to get a fallen nut or to catch an insect. Fallen limbs and sticks must be removed in order to procure a nut or a root. These acts are not entirely unlike moving a bolt, button or hook, or pulling a string or plug. If these are new to him and he manages to operate them, then the whole matter becomes highly favorable to the mental capacity of the monkey.

Just how much these limitations have affected the natural reactions of the monkeys it is quite impossible to determine. It is safe to say that such tests as have been made do not give an unduly favorable view of their capacities. In any event I present here the results obtained under the conditions described, however favorable or unfavorable they may have been.

Apparatus.

The food was put into a box 11x13x15 in., having a door 6x8 in. in the middle of one side of it, and having hinges below so that the door opened outward and downward. A spring set on the inside of the door caused it to fly open as soon as released.

(The numbers on the cut, p. 112, correspond to the numbers of the fastenings described below. Only a part of the fastenings are shown.)

A. *Simple fastenings.* 1. A thumb-button extending in front of the upper right-hand corner of the door. It must be turned upwards or downwards about thirty degrees to release the door.

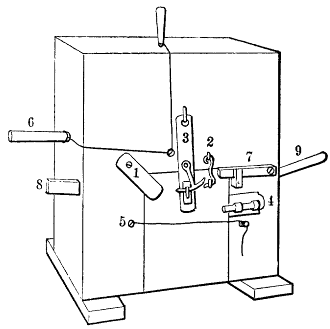
2. A small hook fastened to the box above the upper right-hand corner of the door and hooking into a staple below. This will be referred to as the vertical hook.

3. The T-latch. It was a thin plate of iron fastened by a staple above the middle of the door. By means of a slot in the

lower end of the plate it could be hooked over a staple through which was passed one end of a small T attached to the plate. The T came in from the right.

4. Bolt. This was an ordinary door-bolt. Pushing the bolt back allowed the door to open.

5. String. The string was fastened to a short screw to the left of the door, and wound eight times around a nail at the right. The door could be opened by unwinding the string.



6. Plug. A string fastened to the inside of the door passed upward an inch and thence through a hole in the front of the box, and around to the end where it was fastened to a plug. The plug was stuck into a hole in the box. The plug being pulled out, released the string and the door flew open.

7. Lift-latch. A bar having one end fastened to the box by means of a screw extended behind a cleat near the door, while the other end extended over the upper right-hand corner of the door. Raising the end of the bar next to the door two inches released the door.

8. The push-bar. A bar two inches wide and one-half inch thick passed into the end of the box just back of the boards on the front side and extended clear across just behind the upper part of the door. The cleat of (7) was put on the inside of the door and a notch corresponding to it cut in the underneath side of the pusher. By closing the door and drawing the pusher back three-quarters of an inch the door was held fast. When the pusher was thrust back into the box the end of the cleat passed through the notch and the door flew open.

9. Bear-down latch. It was arranged like the pusher except that it had to be pushed downward about two inches to effect a release.

10. String and bolt. A common door bolt at the top of the door and on the inside of the box could be raised by pulling a string at the rear of the box.

11. String and ring. This was the same as five except that

instead of winding the string around the nail a ring hooked over it.

12. Horizontal hook. It was the same as the vertical hook except that it held the horizontal position.

13. Lock and key. The lock was the ordinary door lock. It was placed on the inside of the box. The spring was removed from it that it might require only a minimum of force to turn the key.

B. *Chute-apparatus.* This consisted of a chute five feet long and inclined at about fifty degrees. At the upper end there was a pivoted tilting-board which being tilted allowed a morsel of food to slide down the chute. The wire front of the cage was brought near the lower end of the chute.

14. A string extended from the lower part of the tilting-board into the lower part of the cage, ending in a toggle that kept the end of the string within the cage. By pulling the string, food could be started down the chute. The monkey could reach out from the cage and get it.

15. The string passed from the tilting-board over a pulley 12 inches above the rear part of the top of the cage. The end of the string then passed down through a hole into the cage. By going to the back part of the cage and pulling the string the tilting-board was tipped and the food could be procured as in 14.

16. The string passed down from the tilting-board to the axle of a windlass. The handle of the windlass was in easy reach from the cage. By turning one and one-half times around the board was tilted, and food obtained as before.

C. 17. A string four feet long was tied at one end to a flat paper box. The other end was fastened to a toggle inside of the lower front part of the cage. By pulling the string in food could be procured.

D. 18. Imitation test. The lock and key. Problem—to put the key into the lock, turn it and procure food.

E. *Second positions.* When any suitable form of fastening was first used on one side of the door it was afterwards changed to the other side. These need only to be named as they have already been described in their first positions. They are: 19. Button. 20. Vertical hook. 21. T-latch. 22. Bolt. 23. String and nail. 24. Plug. 25. Lift-latch. 26. Push-bar. 27. Bear-down latch.

F. *Groups of fastenings.* Again these have been described individually and need but to be named. These might be released in any order. 28. Two buttons. 29. Two bolts. 30. Button and bolt. 31. Two buttons and one bolt. 32. Two buttons and two bolts. 33. Two plugs. 34. Three plugs, one being on the top of the box, two buttons and two bolts.

G. *Designs*. The apparatus consisted of two tin cans, each 2x1-2x2 1-2x3 inches. They were covered inside and outside with white paper, and so arranged that a marking card could be slipped in close to one side, leaving its top extending about three inches above the top of the can. On the side of the card facing the top of the box designs were drawn. The problem was to associate food with the box bearing the design.

35. The card on one box had four heavy, black horizontal lines, each one-eighth of an inch wide, two inches long and separated by blank spaces each one-fourth of an inch wide. The other card was blank.

36. A diamond one inch long and three-fourths of an inch wide on one card, the other blank.

37. A box like the one used with the fastenings, but with the back removed. This was placed with the door facing and near the front of the cage. The door of the small box was fastened with the thumb-button. Two large pieces of cardboard, one having a black paper 4x4 inches on the middle of one side were used as signs. In experimenting, providing food was placed behind the door of the small box, the card with the black square was held before the door of that box, between it and the cage, until well seen. It was then moved and the monkey was allowed, if he desired to do so, to move the button and take the food. When no food was placed there, a blank card was used in the same way. A table for the order of feeding was made out previously. The problem was to see whether the monkey would learn to open the door when the card with the black spot was shown, and not to do so when the plain card was shown.

Method of Work.

In experimenting, except with the string and windlass devices, the monkey was allowed the freedom of the large room. When a series of tests were to be made a few grains of rice or a small morsel of bread, banana or apple was put into the box and the door closed and fastened. The monkey was allowed to see this operation, but was never allowed to see me open the door except for an imitation test. By giving quite small baits the experimenting could continue indefinitely without producing satiety. When he lagged in his work or was inclined to sit in the window or to give the fastening up as beyond a monkey's ken, he could be spurred by showing a banana and tapping it against the door of the box, especially if by sleight of hand it was made to disappear from view at that point. Sometimes he was spurred by showing him the morsel in the box, and again by showing him a whole pile of food in the box. He would apparently work harder for much than for little, quite as if he had his price.

A cumulative stop-watch was used, and account was taken only of the time actually spent in trying to open the box. Time was counted no matter whether the monkey was before or behind the box, whether prancing around it or jumping up and down on top of it, so long as he was trying to open it. Some of these efforts were in no wise directed toward the latch. At first he did not appear even to suspect that the latch was a proper point of attack. But this prancing, etc., was, doubtless, effort to him. Much time was spent in shaking the box when the lock was a difficult one. For this reason the box was nailed fast to the floor.

The male only was taken regularly through these tests. The female gave up so quickly and completely with each new fastening, that to get her to move the bar, lock or button it was necessary to half conceal rice about it so that in the effort to procure the food the lock would be moved. Having repeated this several times she would come to associate the moving of the fastening with the opening of the door. Her time results, therefore, are not to be compared with his. Her work with the locks will be described from time to time under appropriate headings.

Only one fastening was given up by the male. That was the horizontal hook when first presented. He had four hours at his command, and spent seventeen minutes in prancing, shaking, gnawing and jumping up and down. This was the first thing presented to him. We shall revert to it later for a further discussion.

After the above each lock and group of locks was presented and he was allowed to work until the door was opened whether this time was long or short. When the door was opened and the morsel of food eaten, the trap was set again, and so on for thirty times, the seconds required for opening the door each time being recorded.

Results.

The following table and curves indicate the quantitative results of the tests with the male.

This table shows the times in seconds required for working the different fastenings including also the chute, and string and box (apparatus 17) tests. The figures designate the number of seconds used in working the device for each one of the thirty times.

The variations in the times for the fastenings are very great. While on the average the second effort required one-fourth as much time as the first, the T-latch required only 1-462. Again it required nearly twice as long to draw the plug the second time as it did the first time. It is plain that the button, bolt, bear-

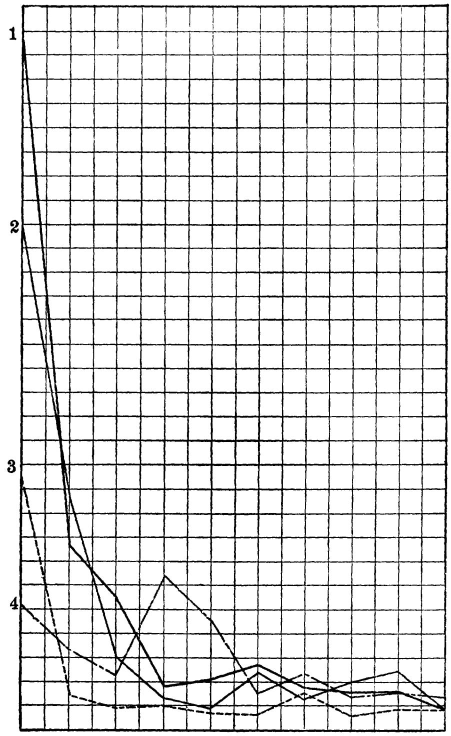
Button	10	1	1	2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Vert. Hook	65	63	3	10	2	40	30	40	67	10	1	25	1	1	59	3	3	9	3	1	20	24	1	1	1	1	1	1	1	2
T-Latch	462	1	1	8	6	52	8	31	1	6	2	12	13	1	3	3	8	3	1	2	4	1	1	2	1	1	1	4	4	4
Bolt	1	5	1	1	1	1	2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	2	1	1	1	1	2	1	1
String & Nail	80	5	3	6	7	7	6	7	12	6	4	2	3	75	36	76	7	8	9	29	28	7	12	14	30	14	23	27	8	8
Plug	125	266	114	30	14	2	3	1	13	2	3	1	1	5	5	2	3	2	5	3	3	3	1	1	1	1	1	1	1	1
Lift-Latch	15	81	12	4	5	3	2	7	7	7	2	2	3	5	2	2	2	4	2	2	1	2	2	2	1	3	2	1	1	3
Push-Bar	168	5	1	3	4	1	3	6	2	1	4	7	1	7	1	9	2	1	1	1	1	1	1	1	2	1	1	1	1	1
Bear-Down	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	2	1	1	2	1
Rear-String	46	6	1	4	3	2	3	1	1	1	1	1	1	1	1	1	2	3	1	5	3	1	3	1	3	1	1	1	1	1
Ring & Nail	213	15	22	9	7	14	47	8	5	7	3	7	1	5	1	1	1	2	1	1	1	1	1	1	1	1	1	1	1	1
String-Box	25	57	4	5	4	7	6	5	5	7	3	3	4	5	6	5	4	10	7	6	4	4	5	5	3	4	6	4	3	4
Hor. Hook	32	4	9	2	2	1	1	1	3	2	1	1	4	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Chute Str. below	3	3	2	2	5	3	6	1	3	1	2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Chute Str. back	366	6	218	18	4	79	7	2	3	1	10	1	1	5	2	1	2	2	2	4	1	1	1	1	2	2	1	1	1	1
Windlass	672	127	21	16	6	5	12	10	4	9	10	3	4	2	2	14	2	9	2	2	2	8	7	9	18	3	3	3	7	6
Lock & Key	100	9	54	32	104	5	6	3	3	3	3	9	1	1	8	1	2	1	2	2	1	1	1	1	2	1	1	1	1	1
Aver.	140.2	38.5	27.5	9	10.3	13.2	8.5	7.4	7.7	3.9	3.1	4.6	2 5	6.9	7.7	7.2	2.5	3.5	2.4	3.7	4.4	3.5	2.5	2.6	4.1	2.3	2.8	3.1	2.1	2.2

down latch and chute with string below, were the easiest for him. Some, as the vertical hook and T-latch, were hard to learn, but easy to manipulate when once learned. Others, as the string-nail, string-box and windlass were difficult to learn, and were of a type that required considerable time for manipulation even though well learned.

The results of the first ten tests with all the different fastenings are shown in Curve I. The seconds are shown on the ordinate by half millimeters. This curve is not, of course, a final representation of the general curve of learning by trial, as it is based upon only seventeen cases (seventeen kinds of locks). However it corresponds well with the curves found by others, and is doubtless of the same general character as one that might be obtained from several hundred cases with the same apparatus. One sees here the rapid decrease of time at the first. The minimum of time is practically reached at the tenth trial, as the table shows. With the individual locks there were considerable relapses at times after the tenth trial. The average time, however, soon becomes measurably regular. The frequent record of single seconds in the table are to be understood as indicating that the animal went to the fastening and released it, with his characteristic speed, with a single movement. The time was often less than one second.

Second Positions of the Locks.

Nine of these locks were put on the door in first and second positions, that is, on the right and left sides of it. The averages of the times required for moving the fastenings, when in



first positions, are given here in the upper line of figures. The averages for the second positions appear in the lower line.

114.1	47.5	15.2	7.2	4.6	12	6.2	10.5	11.7	3.9	2.1
52	7.4	4.8	5.8	4.5	3.2	7.4	2.5	3.8	4.2	3.4
5.7	2.7	10.7	12.1	19.9	3.1	3.2	2.7	4.5	6.6	4.5
2.5	6.3	6.3	6.4	2.8	3.3	2.5	6.1	3.8	2.4	1.7
2.4	2.7	4.3	2.7	3.5	4.3	2.2	2.3	(See Curve 2).		
1.9	3.2	3.1	3.5	4.0	3.5	2.1	1.7	(See Curve 3).		

Curves 2 and 3 show respectively the relative times for working these nine fastenings (see apparatus 19-27) in the first and second positions for the first ten trials. On the average about one-half as much time was consumed for second positions as for first positions. Here the problem seems to have been to learn that the lock was in a new place and that it was necessary to work it in a direction opposite to that used in its former position. It is significant that on the average when these nine locks were in the first position the second manipulation re-

quired nearly one-half as much time as the first manipulation required; while when in their second positions the second manipulation required only a little more than one-seventh as much time as the first one did. While the new position and direction were very troublesome, probably because they stood in direct opposition to the previous associations, when these were once overcome so many remained common elements that their interference was limited to the first trial. After seven trials the time was usually as short as it had been in the thirtieth trial in the first position. It may be assumed that the learning of the new position and direction requires the excess time in the first seven trials over that of the eighth. In that case the mastering of the new position and direction required, roughly speaking, 50—5—2—3—2—1—5 seconds. It is clearly more difficult for the monkey to learn a new device than to master the same device in a new place.

Groups of Locks.

After the fastenings had been learned singly the seven groupings described under apparatus (28-34) were made. In these we find an initial drop in time for the second test of but two-thirds of the first time. This drop is much less than that for single openings. The average time for moving the group of locks the first time is, however, much less than the average time required for learning the single locks. The minimum time is practically reached in the sixth instead of the tenth trial. The averages for the groups are 25.5, 16.5, 11, 31.5, 12.9, 7.3, 11.1, 6.4, 7.4, 6, 9.6, 8.3, 10.6, 9.3, 4.4, 5.7, 6.3, 5.7, 3.7, 7.0, 7.9, 12.7, 6.4, 5.1, 7.4, 5.1, 9.4, 10.1, 5.1, 6.0.

Curve 4 shows these results graphically for the first ten tests. The great rise in the curve for the fourth trial was due to a confusion over the group of seven fastenings. In these cases the method of opening each part was already known; the problem was to get to all the parts and to know what ones had not yet been moved. The time could not be reduced so much as when single fastenings were used, since it was necessary for the monkey to go to different parts of the box in order to reach the several locks. When the group consisted of two or three fastenings the monkey soon adopted a regular routine which he rarely failed to follow.

Experiments with Designs.

Three hundred tests were made with apparatus 37. It was assumed that if the monkey learned to associate the card having the black center with the securing of food and the blank card with not securing food, he would turn the button when

the former was presented, and would not do it when the latter was presented.

Although each card was presented 150 times no association was formed and the button was moved each time without a moment's delay throughout the 300 tests.

Apparatus 35 was constructed next. The food was put into the box where the card bore the four horizontal bars. The two boxes were then set down in front of the monkey, about ten inches apart. After he took the food, a new morsel was slipped in, sometimes from one hand and sometimes from the other. The boxes were then picked up, passed behind my back and exchanged or not exchanged and then returned to the floor facing the new position of the animal. In these he chose the right box 154 times and the wrong one 146 times. Clearly there was no association of the food with this design. The tests were certainly extensive enough, as will be seen when we examine the results where glasses covered with paper of different shades were substituted for the boxes. The monkey failed most likely because his attention was never caught by the marks upon the card. The blank card on the box not having food placed in it, was cut away leaving a strip projecting upwards about three inches. The other box was surmounted with a large square card. Three hundred-eighty tests were made as before of which 242 were right and 138 wrong. Afterwards two vertical strips each about one inch wide and three inches long, one colored blue and the other green, were used with 38 right cases to 52 wrong ones. Glasses were now substituted for the boxes. One glass was covered with black paper and the other with white. Food was put in the one covered with black. The result was that he chose in the following order, + standing for right choice and — for wrong: + — + — + — — + + — +. Though many more presentations were made, there were no more wrong choices. Here we have the association fixed very readily when the apparatus was such as really appealed to the animal.

With the female 50 tests were made with the boxes. One of the cards was blank while the other bore a large black diamond. Thirty of the choices were wrong and only twenty right. But again when paper covered glasses were substituted we have: — + + — — + + + — — + — + + + + + + + + + +. There having been no errors in the last ten choices I considered the association perfected. With considerable justice, it can be inferred from the experiments with the covered glasses that the association with the preceding designs, if it was ever to have been formed, would have appeared with the opportunity given.

Comparison of the Results of the Present Experiments with those Obtained by Dr. Thorndike.

In these experiments, as in Dr. Thorndike's, there appeared no case that could be interpreted as reasoning in the higher senses of that term. When the box was set for the male the first time he went to it and pushed rapidly and violently right and left up and down around the door. Where the edge of the door projected a little beyond the level of the box it was both gnawed and clawed. After working there awhile he chased around the box and went at it again. Then the box was shaken and attacked again. The edges and corners of the box were jerked and gnawed. This was continued for seventy seconds, when he stopped and engaged in a short "flea-hunt." This was followed with alternate work and rest throughout a period of four hours, seventeen minutes having been used in actual effort. In all this time the horizontal hook had hardly so much as been discovered, and apparently there was no notion that it had anything to do with preventing the door from opening. The female tried this hook similarly, two months later, but could not be induced to do more than look at the box after 102 seconds of trial. After the male had entirely given up, the hook was removed and the button was then put on. He opened the box now in ten seconds, but the opening was a mere accident of the general scramble. However, the table will show that he profited by his fortunate accident. In moving it the second time he did not deliberately put his hand on the lock. He seemed to think that scrambling at about a certain place would get the door open, and so he scrambled there. After the eighth trial he put his hand every time directly on the latch and moved it. Throughout the experiments we find what Dr. Thorndike designated as "gradual learning by a gradual elimination of unsuccessful movements and a gradual reinforcement of the successful ones." In this way, however, effort came to be fairly well directed even on attacking a new lock.

Special movements with "directness (which reminds one unavoidably of human actions guided by ideas") appeared in the course of the monkey's efforts; for example, in the application of the teeth where the hands failed, and again in the substitution of hands for teeth where a fastener had been learned and moved several times previously with the teeth, but might be moved more readily with the hands. Dr. Thorndike noted substitutions of this kind with the *Cebus* monkeys. Some of the lower animals under similar circumstances never make substitutions but follow persistently the first accidental method and association. Presumably the animal which can make such a

substitution has a psychic life superior to the one which cannot do it.

Methods of Learning.

(1) *Learning by trial.* I have already mentioned learning by trial more or less definitely directed, fortunate accidents, recollection of these acts, elimination of the useless efforts, substitution of more appropriate methods of accomplishing the end in view.

(2) *Learning by imitation.* Only one opportunity was made use of for getting an imitation of an act performed by the experimenter, and in that case it failed. After the door of the box had been opened as many as thirty times with the key so arranged that it could not be removed from the lock, it was removed and placed near the door. The monkey did not so much as seem to discover that the key was the object that he had just been using. It was picked up both with the hands and with the feet apparently as a mere accident. I then picked it up and opened the door with it fifty different times with the monkey only two feet away and looking on. There was absolutely no effort at imitation. Imitation of one animal by the other, however, was more successful. I tried on four consecutive days to teach the female to work apparatus (6). In order to get her food she had to pull a plug out of the end of the box. After 5½ minutes of trying she gave it up, hardly so much as discovering the plug. But she gnawed the string in two several times. Rice was then put into the hole around the plug, and even out on the plug itself. In the course of an hour I succeeded in this way in having the plug drawn seven times, but only once without rice. She seized the plug with her teeth near where it entered into the box. It was chewed and pulled right and left until it finally fell out. Except in one instance the drawing of the plug did not in the least seem to be associated with the opening of the door. On three other days the experiment was repeated without putting food on the plug. It was pulled right and left but was never drawn. Further, she became so thoroughly discouraged that she would walk away from the box without effort as soon as she saw that the plug was the mode of fastening, even though she had seen rice and other food put into it. At this juncture the male was turned out of his cage. He went immediately to the box, she following some four feet away. Knowing the trick perfectly he seized the end of the plug with his teeth and removed it. I set the box again. This time the female rushed to it, seized the plug by the end as the male did, and procured the food. This she repeated immediately eight times in exactly the same way. On succeeding

days she removed the plug as a part of a combination lock on the same plan 130 times.

Recalling that she had failed to work the bear-down lever for opening the box (see apparatus list No. 9), I placed it before her. She rushed up, but missing the plug she sat down. The male passed her, pushed the lever down and procured the food. When the box was set again she worked the lever and took the food in the same way that he had done. She manipulated this apparatus several times immediately and 250 times later as a part of a combination lock. Besides these, once when the male peeped under the bottom of one of the trees the female came and peeped in the same manner. Neither obtained any food, and neither reached under for any. There have been numerous cases where one followed the other when it jumped onto a table or into a window. But such cases of imitation are rather doubtful. It seems to me that the two cases with the box are quite as good examples of imitation as could well be gotten even with human beings. While this is an unusual method of learning on the part of the *rhesus*, the above example seems to me conclusive evidence that it is at least a possible method for him.

(3) *By singling out special elements in a complex perception.* In this there is progress by the singling out of definite points from the general hazy whole that must have impressed the monkeys in their earlier attacks. The horizontal hook at first was no definite part of the whole box, but after all the other fastenings had been worked by them, including the vertical hook in two positions, the horizontal hook was replaced. Now the vagueness had been reduced. The hook, like the other later fastenings, was singled out and attacked directly. Through a great many scratchy movements at it it was moved by the male in 32 seconds as against 1,000 seconds at the beginning which had ended in failure. The female on returning to the horizontal hook moved it in 5 seconds as against 102 seconds with failure at the beginning of her tests. It appears that there was improvement in dealing with fastenings in general. All locks toward the last were attacked with definiteness and as if the animals had a dim realization that they were the proper objects for attack, a thing which was not true at first. This growing capacity to deal at once and with considerable success with a new lock was very noticeable. It is an index of the monkeys' educability and of the road by which they progress. It looks very much like the possession of a sort of general notion fairly well represented by projecting-thing-has-something-to-do-with-it, and so they attacked the projecting thing and not something else. (This and some similar cases will be analyzed in the latter part of this paper.) This is evidence of

educability within narrow limits. If one accepts intellectual evolution he might expect, at least, as a possibility with the higher classes of animals, a small degree of general improvement.

To sum up briefly this phase of my experimental work I find:

1. The monkeys have not reasoned in the higher sense of the term.

2. First efforts require much more time than later ones, the time shortening very quickly. There are frequent reverses after an association has been fairly well fixed.

3. The monkeys are capable of substituting a better for a poorer method of manipulation.

4. In dealing with groups of manipulations they adopt a regular order.

5. The main body of their learning has been by trial and happy accidents, the recollection of these and the elimination of useless efforts.

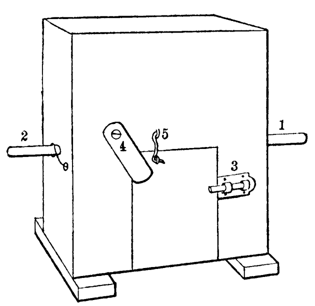
6. The female, however, has learned by imitation.

7. They have shown an increasing ability to pick out the fastenings as the essential point of difficulty in opening the box, and to direct their efforts upon them alone.

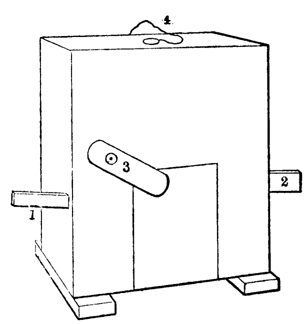
Combination Locks.

The remainder of this paper is based on experiments which, so far as I am aware, have never before been made on monkeys. The first have to do with locks of a more complicated character.

Apparatus.



COMBINATION I.



COMBINATION II.

1. A box like that used in the previous experiments was again employed. The door was fastened by a vertical hook at the upper left-hand corner so arranged that a button near it had to be pushed back before the hook could be moved. But

the button could not be moved until the bolt at the right side of the door was pushed back. And this in turn required the removal of a plug at the left end of the box before it could be moved, and finally a plug at the right side of the box had to be moved first of all. So the only order of removal which could result in success was, right plug, left plug, bolt, button, hook. In the cut this order is indicated by the numbers, 1, 2, 3, 4, 5. Further, this was the only order in which they could be moved.

2. The same box. The fastenings this time, and their order are, bear-down lever at the left, push-in bar at the right, lift-up latch in front, and pull-out string behind. These are all so arranged that when once moved they lock and cannot be shaken back and thus prevent moving the next part of the combination. The cut indicates the plan, and the figures the order, as with combination lock 1.

Method.

Lock 1 was used first. The hook was in reality no additional element since when the hand moved the button the thumb caught the hook, so that except in rare instances, the hook caused no additional movement whatever. When experimenting with the female the hook was left off. Each of the parts of the combination lock had been moved thirty times as a single lock before it was drawn into the combination. The problem was, therefore, not that of learning the movement of the parts but of learning the order of movements which would get the food. Feeding, timing, etc., was conducted as before. In addition an attendant recorded the time and made the notes dictated so that my whole time could be devoted to the observations. The record of the monkeys' movements was kept by calling off the number of the part of the combination attacked, and of the second at which any part was moved. When starting out I set the arbitrary standard that the combination would be considered as learned whenever it was worked ten times consecutively without error. Every effort to move any part out of its regular order was considered as an error. With combination lock 1, 253 trials by the male and 80 by the female were required to meet this arbitrary standard. In evaluating the results the last three tests of the 253 above were omitted. After this each monkey was worked 250 times with the second lock. Neither attained the arbitrary standard with it.

The following tables of times and errors indicate the progress quantitatively. For convenience the 250 tests are thrown into 25 consecutive groups of ten each and the *average time* for each of these groups is given. The errors have been treated in the same way.

TABLE OF TIMES.

First Combination Lock.	Male	34.0	28.3	33.1	14.4	9.5	9.0	9.8	11.3	10.6	8.4	11.9	8.8	9.9	11.4	8.9	9.2	7.4	6.2	11.4	5.9	7.2	7.8	6.1	5.6	6.4
	Female	89.8	44.	31.	16.2	17.5	10.1	6.1	5.1																	
Second Combination Lock.	Male	105.3	80.3	68.7	31.0	40.8	27.0	20.0	22.7	22.0	21.0	15.0	9.4	21.5	22.5	14.3	19.5	17.4	17.7	12.5	12.1	12.9	13.9	13.0	12.4	13.6
	Female	100.1	63.2	61.2	77.8	32.5	39.4	16.2	20.6	19.6	23.7	20.1	27.8	23.5	21.3	20.4	22.6	23.3	19.4	18.1	17.7	18.2	20.7	18.7	12.0	19.6

TABLE OF ERRORS.

First Combination Lock.	Male	6.1	5.5	8.4	4.1	2.4	2.3	2.3	3.6	3.5	2.2	3.2	2.1	2.5	3.2	2.2	2.2	1.6	.8	1.9	.7	1.3	1.3	1.4	.7	.4
	Female	11.9	11.8	7.6	3.8	4.1	1.2	.3	.0																	
Second Combination Lock.	Male	26.8	16.5	12.6	7.4	8.8	9.5	6.5	11.4	8.3	7.3	4.0	1.3	6.8	8.3	5.3	6.1	7.5	9.1	4.4	3.9	4.7	5.1	5.3	4.7	5.0
	Female	18.2	12.8	13.8	23.3	12.1	15.7	7.8	7.3	7.8	9.9	9.5	13.3	11.6	10.3	9.4	10.3	9.9	10.8	9.4	9.9	9.3	11.4	9.0	4.3	7.4

The tables show both in times and errors a rather rapid reduction at first and a slower reduction later, along with numerous relapses. It will be noticed that on the second combination lock the twelfth group of 10 tests with the male shows a very great reduction both in time and errors, immediately followed by higher numbers and that this low standard was not reached again. At this twelfth group 7 out of 10 of the efforts were without error. I expected him to reach the standard in the next group, but at this juncture a distinguished friend and visitor came in for a few minutes. The animal took fright, and as it was very late, the experimenting was postponed to the following day with the result above. While the break prevented the monkey from accomplishing his feat it furnished a fine example of the effect of the fright and the fifteen hours' delay on the slow mastery of what was proving to be a very difficult task.

It will be seen that the female reached our arbitrary standard with the first lock in eighty tests. A further comparison of the processes of the male and female here as in the case of many of the other tests will be made later under the heading of "Individual Differences."

In working combination 1 the male went from the right plug by way of the front of the box to the left plug. In doing so he passed by the bolt and button, neither of which could be moved before the left plug was removed. In the earlier attempts he tried these fastenings as he passed by them, later he would raise his hand as if to put it on the bolt, and then as if remembering that it could do no good he would withdraw the hand without touching. This was repeated several times before he came to pass it by without notice.

The female went from the right plug around by way of the back of the box, but for a long time tried the bolt, using the left hand, before starting around. Here again came a course of ten or twelve cases in which the hand was extended part way and then as if to say "no, that does n't come next," she withdrew the hand and went on her way.

Twelve human adults and five children were tested with the same combination lock. Unfortunately no record was kept of the number of their errors. Later, two adults and two children were systematically tested with both combinations. The first and second times, given in seconds for the twelve adults were, 90-5, 45-4, 69-8, 13-3, 11-3, 57-7, 19-4, 300-6, 154-5, 132-7, 20-4 and 7-3. Correspondingly the times for the children were, 112-30, 216-21, 184-31, 65-13 and 45-7. The monkeys' first two times were, 78-33 and 64-65. The two adults tested ten times with combination 1 reduced their times respectively from 6 and 8 seconds to 1½ and 2 seconds. The

children reduced their times respectively from 80 and 45 seconds to 2 and 2 seconds. The monkeys' show no material change from their first to their tenth trials. Read as above their times are, 78 and 54 for the first and 52 and 90 for their tenth trials. With the second combination the two adults changed from 11 and 36 to 2 and 4. The children changed from 105 and 20 to 6 and 3, while the monkeys changed from 139 and 187 to 110 and 26.

Allowing due range for individual differences it yet appears that the results obtained with the children resemble the results obtained with the monkeys more than do those obtained with adults. The essential method of learning these combinations on the part of human beings is just the same as on the part of the monkeys, that is, trial, the remembrance of accidental successes and the dropping away of superfluous efforts. The nature of the locks gives little opportunity for reasoning one's way to success.

When the human adult attacks the box he looks at it and attempts to reason about it. If he tries the wrong latch first he is slow about attacking it again. He usually tries to move a latch by the use of strength enough to break it. If urged not to break it, he stops and tries to reason again. When he gets one latch moved and takes hold of a wrong one next he is liable to return and try to replace the one already moved. But when once through he usually remembers the road to success and retraces it very rapidly. By the third time he has practically reached his highest speed.

The child tries immediately, and, like the adult, plays havoc with the apparatus if it is not equal to his strength. He holds on, where he tries first, for some seconds. Then he looks bewildered, tries something else as before, draws a long breath and looks helplessly to you for directions. Urged to go ahead he may try the same thing over. Finally he succeeds in moving one part. He stops and looks up, apparently with a feeling that it came too easy or as if he feared that he had broken the machine. Errors persist longer with him than with the adult, and his time is reduced more gradually.

But when the monkey attacks the box he does not stop to think about it. He tries one thing, then another and another in quick succession. One part worked he tries all the others, though he may labor on one at times, and try often to move parts that have already been moved. If he fails to move the first part of the combination he may go again and again over the second, third and fourth parts. As he does so he comes to make no real effort at any one of them. Each is given only a passing trial or a mere touch.

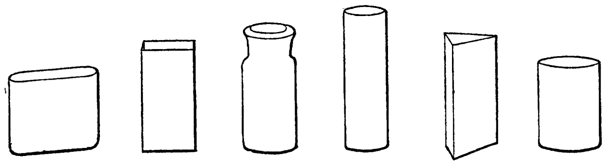
The short-circuiting process by which speed and skill are

finally attained is of great interest. Take for example, a record of the male's second effort to work the second combination. The order for success should be (1), (2), (3), (4). The following series of figures shows the course of the monkey's efforts. His successes are shown in heavy faced type when he got it right. **1,** 3, 2, 4, 3, 2, 3, 2, 4, 2, 1, 2, 3, 1, 3, 2, 1, 3, 4, 1, 3, 2, 4, 1, 4, 2, 3, 3, 2, 3, 1, 4, 2, 1, 3, 2, 4, 1, 3, 3, 2, 4, 1, 3, 2, 3, 1, 4, 2, 1, 4, 2, 1, 4, 2, 3, 2, 1, 4, 1, 3, 4, 2, 4, 1, 3, 4, 4, 3, 4, 3, 2, 4, 1, 3, **2,** 1, **3, 4.** Here we have 75 errors. It will be seen that latch (2) was tried 19 times before it was moved. The latch moved easily enough, but he either pushed it in the wrong direction, or made no real effort at it. The above is a record of his second trial and is one of his longest. The twentieth trial reads, 2, 4, 2, 3, **1,** 3, 4, **2,** 3, 4, **3, 4.** This latter is a typical case for more than a hundred trials. Later, between the 110th and the 120th trials, he made no mistake in 7 out of 10 times that he opened it. In the first trials he seems to have forgotten how to move (2), which was the push-bar. When that was moved he usually finished very quickly. But take a case like the last one quoted above, how is the monkey to know that the extra pulling is not a part of the game, since he can see no immediate advantage in any of the movements except the last one? Yet he does manage to get the usless efforts out. Take his 86th trial. It reads, 2, **1, 2,** 4, **3, 4.** Here he has improved over the 20th trial by dropping out the useless 2, 3, and 4 before 1; the useless 3 and 4 before 2 and one useless 3 before 3. This short-circuiting is not a rational process. It, like most other cases of his learning, is accomplished by the method of trials. In his fumbling and prancing he went at odd times from 1 to 2 without stopping at 3 or going around to 4. This then brought the result without the intervention of useless trials. Gradually, then, these successes were associated until at last they were firmly fixed in their correct relation. The order 3-4 was the first part learned. The order 1-2 came second, and the order 2-3 was learned last.

Summary. In the course of this group of experiments we have met two excellent cases of inhibition on the part of the monkeys. We have found that they are able to deal with complex and difficult pieces of apparatus; and that their learning by trial and happy accidents is not limited to the learning of single and simple devices. Lastly, the experiments have furnished us an opportunity to compare animals and human beings in their reaction to the same apparatus under much the same conditions. I believe that a series of experiments with this and similar complex apparatus could be made on children and monkeys under conditions quite the same, and would furnish mate-

rial for one of the most interesting chapters in comparative psychology.

Form Tests.



In order to test the monkeys' ability to discriminate forms, I arranged six small vessels each with a capacity of half a pint. These consisted of a wide mouthed bottle, a small cylindrical glass, an elliptical tin box, a triangular paper box, a rectangular paper box and a tall cylindrical can. These were papered inside and outside as neatly as possible with white paper. This particular size of vessel was selected because of the convenience of feeding. Larger vessels however might have been used. In making experiments, these were set at regular intervals on a board 1x7 inches and 5 feet long.

In the first series of tests a bit of food was put into the rectangular box (later in other forms of the set). The board was then placed on the floor at right angles to a line from the monkey to the middle of the board. The operator stepped back a short distance in this line and awaited the monkey's selection of a form. Then the board was placed on a table where the forms were rearranged and a bit of food again put into the rectangular box, after which the experiment was repeated as before. After each test the boxes were rearranged according to a schedule previously made out to insure that consecutive presentations would leave the food box neither at the same place on the board, nor between the same forms. The monkey, being free in the large room, moved about a great deal, so it rarely happened that the board was set down twice consecutively in the same place.

The assumption was that the keenness of the monkey's appetite would induce him to go at once to the form containing the food, if he could distinguish it, and waste no time looking into empty boxes. It would thus be possible by noting his success or failure to judge his ability to distinguish the different forms.

On the average the forms were set down nine or ten feet from the monkey. When he became over enthusiastic it was impossible to get it down more than five feet from him. Whenever he was as close as five feet I set the board down slowly, backing off and compelling him to follow for some distance.

This gave him opportunity to survey the vessels and make his selection. If set down too near him he would rush to the nearest vessel, making no choice whatever. A typical case would be like this: the monkey ten or fifteen feet away, "flea hunting," continues until he hears the board strike the floor, when he rolls up onto his feet and dog-trots over toward the board. The eyes are blinked toward the board as a whole or moved so as to sweep the entire length of it in the fraction of a second. At a distance of from six to ten feet it becomes apparent which vessel he means to try first. Occasionally, however, when the association has been established fairly well, he may come within two feet of the board in the direction of a wrong vessel, and then, as if noticing the mistake, make a rather sudden curve and go to the correct form. Until the association had been pretty thoroughly established the monkey inspected the other vessels after getting his morsel. But when the matter had been well learned he took the food and trotted away immediately. When he came up directly to the wrong vessel, he looked into it with hand up-lifted to take the food. Not finding it he looked into the vessel next to this one. This looking into the vessel was continued until the food was found. If food were in the vessel standing in position (1) at the left of the board for example, and he should come up to the one in position (2) he might go next to (1) or might pass along the six forms in the order, 2, 3, 4, 5, 6, 5, 4, 3, 2, 1. On the return movement (5) was looked into in the earlier experimenting, but later, the return would omit (5) and occasionally both (5) and (4). The tall cylindrical can appealed so little to the monkeys that they rarely went to it directly, but in case of a wrong start, if the can came in between the vessel first looked into and the vessel containing food, it was examined like the rest.

TABLES OF ASSOCIATION WITH FORMS.

(*Male.*)

FORMS USED.	FOOD IN RECT. BOX.			FOOD IN GLASS.						FOOD IN ELLIP. CAN			FOOD IN TRIANGULAR BOX.			
Triangular Box	6	3	1	1	2						4		**3**	**11**	**26**	**25**
Glass	1				**11**	**19**	**26**	**29**	**30**	16	5	1	5	3	1	1
Bottle					3	10	2			1			1			1
Tall Can							1			2						
Elliptical Can	4			1	4	1	1	1		**10**	**21**	**29**	15	9	1	2
Rectangular Box	**19**	**27**	**29**	28	10					1			6	7	2	1

(*Female.*)

Forms.	Food in Rect. Box.		Food in Glass.			Food in Ellip. Can.			Food in Triangular Box.		
Triangular Box	6	2	2						**7**	**10**	**25**
Glass	8		**2**	**22**	**28**	27	14	3	3	1	
Bottle	2	1		2		1					
Tall Can											
Elliptical Can							**7**	**26**	12	11	4
Rectangular Box	**14**	**27**	26	6	2	2	9	1	8	8	1

The results of the tests have been divided into groups of thirty each. The numbers in each of the vertical columns in the tables above indicate how often the animal went directly to the form in question in the thirty tests. No account is here taken of the forms looked into after the one to which the monkey went directly. Thus if the food was in the glass he may have first gone to the bottle and then to several other forms before going to the glass. In such cases these tables take account only of the look into the bottle.

The original notes were fuller, however. An attendant recorded the results for me. If the animal went directly to the box containing the food this was noted as a success. If, however, he went first to some other form, a note was made not only of the form approached first but also of what other forms were looked into before finally coming to the one containing the food.

The male was fed consecutively 90 times from the rectangular box; afterwards 180 from the glass; 90 from the elliptical can, and 120 from the triangular box. The female was fed from the rectangular box, glass, elliptical can and triangular box respectively, 60, 90, 90 and 90 times. Testing was always continued until the animal could make from 25 to 30 correct choices out of 30 trials. In some of the size and color tests to be mentioned later, when as many as 300 tests were made with no signs of improvement in the association, the experimenting was discontinued without this standard having been reached.

When the food was placed in the rectangular box which was the first form used, the association was practically perfect for both animals in 60 tests. With the male when the food was changed to the glass the table shows the old habit practically broken in the second 30 of trials and the new habit practically perfect in the fourth 30. The female perfected her association with the third 30. When food was put into the elliptical can the old habit was broken up in the second 30 and the new was perfected in the third. In changing to the triangular box the old habit was broken in the first 30 and new was formed in the

third. With the female the change from glass to elliptical can revived the first association so that the rectangular box was chosen 2, 9 and 1 times respectively in the three thirties. When the food was changed to the triangular box this first association was again strongly revived, the rectangular box being chosen 8, 8 and 1 times. A similar revival occurred with the male when the food was put into the triangular box.

Sometimes the changes or breaking of the previous associations were very sudden. In the table it will be seen that the male made 19 correct choices in the first 30 tests when the food was put into the rectangular box; and, again, 10 correct choices when the food was put into the elliptical can. These would appear less abrupt if the results were arranged in groups of 10 each instead of groups of 30 each. In the first of the cases just mentioned the male made no correct choices in the first 13 tests. In the second case 6 of the correct choices were made after the fifteenth test.

The superiority of human beings in breaking such an association may be seen in the fact that my assistant who helped to change the position of these forms and who put the food into the boxes, in all of the above changes, only once or twice threw the food into a box used in a previous series, while the monkeys returned to their former associations from 21 to 36 times. Their success in establishing associations with these forms was greater than with the sizes and colors tried later.

If one takes into account the total number of forms looked into by the monkeys instead of counting only the number of times that they went directly to the wrong one, as I have done in the preceding table, the errors appear to be a little more than three times as numerous. To illustrate, in the first 30 tests for the male as shown in the preceding table, when food was put into the rectangular box, he made 11 wrong first choices, but he looked 25 times into wrong boxes. When the food was put into the elliptical can, in the first 30 tests, while he made 20 errors by direct choice he looked 61 times into the wrong boxes. This method of counting errors, while suggestive, does not seem to me to be the best method of evaluating the reactions. If the food is in the glass and the monkey comes from a distance and goes directly to the bottle, he clearly makes a mistake. He has selected the wrong form and should be credited with one error. If the rectangular box stands next and he peeps into it this ought not to be counted an error since it is not a choice. The box stands next and he goes to it for no other reason than that it is next. This difference between a *choice* and a *mere look* was nicely illustrated with the tall cylindrical can. While it was rarely ever chosen, as the preceding tables show, it was nearly always looked into if a

mistake was made and it came in the line of search. In all the tests following, in sizes, colors and numbers no accounts will be taken in the tables of this extra looking into vessels.

With the male when the food was put into the glass a confusion arose between it and the bottle. This is shown in the table by the 3, 10 and 2 times that the bottle was chosen. The fact that the two forms were somewhat alike will account sufficiently for the confusion.

The monkey's problem in these tests was at first to associate the food with a special form. When the food was afterwards put into other forms the problem became more complex, and the monkey passed through three distinct stages in dealing with it: (1) a series of artless returns to the forms previously used, (2) a stage of confusion in which there was a conflict between the old association and a dim realization that the food was not to be found where it had been before. In this stage the monkey was about as liable to go to one of the six forms as to another. The duration of this period was not always long enough to show in a table made out by thirties. (3) A stage of progress toward perfection in the new association. The processes at this stage seem to follow the same order as where there were no preceding associations to be broken up.

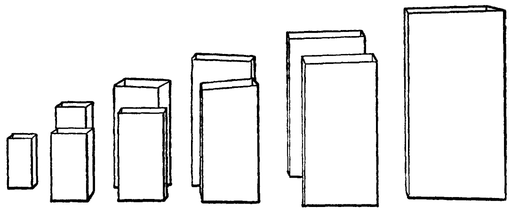
That the animal's choice was not based upon the sense of smell but upon the visual recognition of the form was very apparent. With one exception, when the monkey was searching for food, the head was turned so as to bring the eye over the top of the vessel. In the course of several thousand tests of this general type the monkey only once seemed to try to locate the food by the sense of smell. All discussion of this question, however, is reserved for a section on "The Sense of Smell."

Summary. Restating briefly I may say that the monkeys are able to discriminate these forms and to associate food with them consecutively. The associations are not formed by a single trial, but come about more or less gradually through much repetition. It is easier to form an association *de novo* than to break an established one and form a new one. The necessity of forming a new association induces a revival of former associations of the same general kind. The learning process, upon the whole, is still that of trial, happy accident, recollection of the fortunate movements, and an elimination of the useless ones.

SIZE TESTS.

The tests for discrimination of sizes were made on the same general plan of those for forms. Instead of the boxes there used I made six rectangular paper boxes having an altitude

twice one side of the base. Their heights were 2, 3, 4, 5, 6, and 7 inches respectively. In the cut, which was made from a photograph, these boxes are shown as the smallest and the largest



together with the four others in the rear. The first box was too small to be used for feeding and the last was rather tall. For this reason the monkeys were fed only from the four intermediate boxes. We shall speak of these size-boxes by the numbers 1 to 6 beginning with the smallest.

Although many more tests were made with the boxes of different *sizes* than with the *forms* the association never became so definite. The *forms* used in the preceding series must have been much more readily distinguished than are the *sizes* used here, for while the male made the four associations of *form* with 480 tests, 1,080 tests with the *sizes* sufficed only to induce a general grasp of about such and such sizes. The female was tested with only a part of these sizes, but with these she showed greater ability than the male though she made about the same confusions as he did.

In the form tests they had to associate their food with a particular geometrical figure and had only to look for that one particular thing. No other form needed any special attention. In trying to select the correct size, however, if the monkey was to succeed in choosing rightly, he must compare before he could be more than measurably sure of choosing correctly. Probably the only alternative to such a method was for him gradually to fix upon an absolute size and thus resolve his problem back toward that met in the form tests, though this would be by no means easy. It may be that this is what the female did and thus secured better results than the male did.

The blunders of the monkeys in these size tests are quite interesting in themselves. When the food was put into box 3, 3 and 4 were frequently confused; while 3 and 2 were confused much less frequently, even less often than 3 and 5. The same thing appears throughout the tests. When the food was in 5, 6 was chosen more frequently than 4, even when 4 was much nearer the size of the previous feeding form. This can sometimes be accounted for by the order of the tests. For example, when the food was changed to 2, after being in 3 and 5, on account of

the previous association with these larger forms, 5 and 6 were chosen oftener than 1. But the tendency cannot always be explained in this way. When the food afterward went over to 4, 5 was chosen more frequently than 3; and 6 more frequently than 2. Why should the confusion be always with the larger forms? My first thought was of the psycho-physic law. While the altitudes of the boxes were 2, 3, 4, 5, 6 and 7 inches, the lateral surfaces presented to the monkeys as they approached were, 2, 4½, 8, 12½, 18, and 24½. Thus, while box 1 presented a surface less than ½ of box 2, box 3 was nearly ⅔ of 4; and 5, nearly ¾ of 6. The proportional difference constantly grows less and less. If volumes are considered, the variation is still greater. Then the ratio of box 2 to box 1 is much greater than that of box 4 to box 3, or the ratio of difference decreases as the larger boxes are compared, so that the stimuli for distinguishing boxes become less and less. This confusion of the larger forms would then be in harmony with the psycho-physic law of stimulus and sensation. The less the proportional difference the greater the confusion of forms. As a control test, I constructed a series of boxes varying in volume by the set ratio twenty-one tenths beginning with the first box as before, 1 x 1 x 2 inches. The other boxes like this one were made twice as high as wide. The sixth box was so nearly the same size as the sixth in the former tests that the old 6 was used again. The volumes of these boxes were:

Old series:	2	6.75	16.	31.25	54.	87.75
New series:	2	4.2	8.82	18.52	39.2	82.31

These forms are represented in the front row in the cut along with the largest and smallest boxes as before. It was assumed that if the monkeys selected according to the psycho-physic law in this matter they would confuse the feeding box each time equally with larger and smaller boxes. Here again, however, the confusion was with the larger forms. It may be that the tendency was to choose the larger forms simply because they were more conspicuous. Possibly, in monkey logic, it may appear that the larger forms should bear the most fruit; so that by choosing a larger form more food would be obtained, and that desiring to run no risk of missing food they went to the larger boxes.

Summary. These tests reveal what seems to be an ability either to compare sizes roughly or to fix upon an absolute standard of size. If one recalls the experience with the designs it will not seem probable that this association was based on any accidental markings or other minor peculiarities of the boxes. Their choice does not seem to have been made in any perceptible degree according to the psycho-physic law. Besides these this

study reveals nothing not already shown in the experiments with the *form tests*.

Discriminations of Color and Shade.

The presence everywhere in the animal kingdom of colors, ranging from the most modest to the most brilliant, protective coloration in animals, and the display of colors in animal courtship, would suggest, at least, the possibility that animals not only perceive colors but have preferences among them.

But, except for insects, very little has yet been done to give experimental proof of color perception, and almost nothing to show that the apparent cases of color perception and preference are not based upon discrimination of light and shade rather than genuine color perception. If, for example, an animal shows a preference for a light yellow over a dark blue, one must make sure that he is not merely choosing what appears to him to be a light in preference to a dark gray, instead of one color in preference to another.

Lubbock's experiments upon insects are not complete in that he failed to take this factor into account. In his experiments with ants, for example, he put strips of colored glass over their nests, and after a given time counted the ants congregated under each of the strips. *Formica fusca* congregated under these strips in the following numbers: red, 890; green, 544; yellow, 495; violet, 5. After numerous experiments with bees he says: "It seems to me that the preceding experiments show conclusively that bees prefer one color to another, and that blue is distinctly that favorite." Wasps also, "are capable of distinguishing color, though they do not seem so much guided by it as bees." That these insects have color preferences is probably true, but the experiment would be more conclusive if Lubbock had excluded the possibility of the results having depended upon differences in brightness. The choice of red over yellow and violet may have been due to a difference in the brightness rather than in the color of the glass strips.

The probability is very strong that birds recognize colors and have preferences among them; but, so far as I know, definite experiments are almost entirely wanting. About color perception in the higher classes of mammals there has been a great deal of surmising and guessing. It is said, for example, that horses delight in bright colored ornaments on their harness. But there is no definite proof of it. Mr. Cornish well summarizes current opinion on this matter. He says: "Domestic animals, which see bright colors other than green in large masses more frequently than wild ones, might be supposed to exhibit the consciousness of such differences in the most pronounced way. Yet it is next to impossible to cite an

instance in which a dog exhibits curiosity as to color, or identifies an object by its hue. The writer has seen a setter refuse to retrieve a black rabbit because it apparently thought its master had shot a black cat. But a house-living dog shows no preference for a red carpet or rug over a blue or variegated one, and expresses no surprise or curiosity whether its master wears a red uniform or a black evening suit. Domestic cattle are so much affected by violent contrast of white and dark that the presence of a black, white or very clearly spotted animal in the herd sometimes results in calves being thrown of the same colors or markings. But though red is said to irritate a bull, and to excite hunters by association of ideas, the latter statement rests partly on surmise. They are equally excited by the sound or sight of hounds, or of a number of riders, whatever the color of their coats. None of the cats, whether wild or tame, show any partiality for bright hues; and among all the strategems used from time immemorial by hunters, the use of color as a lure for quadrupeds is notably absent. Many birds, on the other hand, have a marked preference for bright colors, and exhibit strong curiosity when unusual tints are shown to them. Among the less known examples is that of the red-legged partridge. These birds abound in the lower spurs of the Lesghian Mountains, near the Caspian, and the native hunters use a device for killing them based on this æsthetic preference of the partridges. By the door of nearly every house stands a wooden frame, on which canvas is stretched, covered with daubs of brilliant colors. This the shooter carries with him, and sets up in front of him as soon as he has discovered a covey. As soon as their attention is attracted he waits behind the screen, until the whole covey run up to within shot, and then fires through a loop-hole in the center of the screen. The Russian government has now forbidden the use of these colored lures, as the birds were being exterminated. It is probable that the idea of their use was first suggested by the interest the birds took in the carpet frames set up outside the houses for weaving the brightly-colored Shusak rugs."[1]

I am aware of but two series of experiments on mammals, except the *genus homo*, to determine their ability to discriminate colors. One of these was made by Mr. Elmer Gates. He experimented with dogs, using small pans of various colors and shades, and colored metal plates. Food was put regularly under pans of a given color or shade, and these were distributed among a great many other pans of different colors and shades. The metal plates were so arranged in a hall-way, through which the dogs passed, that they had to step on them. Plates

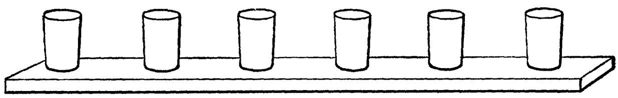
[1] Animals at Work and at Play, C. J. Cornish, p. 130.

of a given color were connected with induction coils, so that whenever the animals stepped upon them a slight shock was received. Mr. Gates believes he has shown in these experiments that dogs not only perceive colors but they also perceive very small differences of shades in colors.[1] The other experiments were made by Mr. Garner. He says: "In order to ascertain whether monkeys had any choice of color or not, I selected some bright-colored marbles, candies, balls, bits of ribbons, etc, I took a piece of pasteboard, and on it placed a few bright-colored bits of candy, which I offered to a monkey and watched to see whether he would select a certain color or not. In this experiment I generally used two colors at a time, and changed their places from time to time in order to determine whether he selected the color by design or accident. After having determined which of two colors he preferred, I substituted a third color for the one which he cared least for, and continued thus until I exhausted the list of bright colors. By changing the arrangement of the objects a great number of times it could be ascertained with comparative certainty whether the color was his preference or not. I find that all monkeys do not select the same color at different times. But I think that bright green is a favorite color with the Capuchins, and their second choice is white. In a few cases white seemed to be their preference. I have sometimes used paper wads of various colors, or bits of candy of the same flavor rolled in various-colored papers. They seemed to choose the same colors in selecting their toys. I have sometimes used artificial flowers, and find that as a rule they will select a flower having many green leaves about it. It may be that they associate this color with some green food which they are fond of, and consequently that they are influenced by this in selecting other things. I kept a cup for a monkey to drink milk from, on the sides of which were some brilliant flowers and green leaves, and she would frequently quit drinking the milk to play with the flowers on the cup, and seemed never able to understand why she could not get hold of them. In one test I had a board about two feet long, and laid a few pieces of pink and white candies in four places on it. The monkey took the white from each pile before touching the pink; except in one instance it took the pink piece from one pile. I repeated this test many times. In another test I took a white paper ball in one hand and a pink one in the other, and held out my hand to the monkey, who selected the white one nearly every time, although I changed hands with the balls from time to time. These experiments were mostly confined to the *Cebus*

[1] Monist, July, 1895.

monkeys, but a few of them were made with *Macaques*. They seem to be attracted generally by all brilliant colors, but when reduced to a choice between two, such seem to be their tastes."

The question has seemed to me of sufficient interest to justify a more thorough investigation, and I have made a somewhat extended series of experiments (6,700 tests) under as favorable conditions as I could command. The general form of the experiments resembled those on form and size. I have used five pieces of apparatus, four of which are described below.



(1) A board 1x7 inches and five feet long. At regular distances six holes were sunk in the board, each large enough to admit freely the bottom of a cylindrical glass. This board held the glasses firmly and yet allowed them to be taken up easily and interchanged. The convex surface of each glass was covered with paper. A bright blue, a bright yellow, a dark red, a light green, a dark gray and a light gray. The brightnesses of these colors were determined by Rood's "Flicker method," with the result that the red was found to be as bright as a gray produced by mixing 35° of white with 325° of black, the blue as bright as a gray with 50° of white and 310° of black, the green as bright as a gray with 117° of white and 243° of black, and the yellow as bright as a gray with 215° of white and 145° of black. The dark gray corresponded to 90° of white and 270° of black; the light gray, to 167° of white and 193° of black.

(2) The board and glasses were the same as before, except that the two glasses covered with gray paper were not used. Each of the above color glasses was used singly with three other glasses covered with gray paper having the same brightness as the color with which they were used, determinations being made as before by the "Flicker method."

(3) The board was the same as in (1) but the six glasses were covered with grays of brightnesses indicated by the following degrees of white as determined with the rotating discs 215, 167, 135, 90, 35 and 0. The sixth above was intended to be black, though it was not absolutely so.. It was not measured, but was estimated at not more than two or three degrees of white; for convenience it will be spoken of as if absolutely black.

(4) Nine glasses covered with gray papers having brightness

corresponding to the following degrees of white: 215, 185, 167, 150, 135, 117, 90, 35 and 0.

The method employed in the first series of tests was the same as that used with the forms and sizes. The glasses covered with gray were not used as feeding vessels. The four colors were used in the order blue, yellow, red, green. The main results appear in the accompanying tables which are arranged on the same plan as those under the form tests. The tests have been divided into groups of 30 each. The figures in each vertical column indicate how often the animal went directly to each one of the colors and grays within a group of 30 tests. Again, as explained above, our tables take account only of the glasses to which the monkeys came directly.

It will be seen that the male required many more presentations than the female in order to reach our arbitrary standard of from 24 to 30 right choices out of 30 trials. It will be recalled that the female surpassed the male also in the form and size tests. In the tables for the female we note again the revival of the first association when the third and fourth were being formed. The second association also was strongly revived with the fourth. These revivals were less marked with the male. From these tests alone we cannot infer that the monkeys have a clear perception of the colors, for a discrimination of the differences of brightnesses alone might possibly bring about the results found. The likelihood of the latter alternative is perhaps somewhat strengthened by two relations which appear in the above tables. (1) The association with the yellow was revived more strongly with the green than the association with the blue was. (2) Upon the whole, the dark gray was chosen more frequently with the red and blue than with the green and yellow. It will be seen in the description of the apparatus that the dark gray had a degree of brightness nearer to that of the red and blue than to that of the green and yellow. While the former of these may have been offset by the fact that the association with the yellow was more recent than that with the blue, and the latter may have been made less significant by the fact that the light gray was chosen somewhat more frequently with the darker than with the lighter colors, still there remains the possibility that the confusions named above may have arisen out of a partial or complete failure to perceive the colors as colors.

In order to determine whether brightness or color was the basis of discrimination, four control tests were made. In three of these I attempted to determine whether the monkeys could discriminate grays and colors varying by the same degree of brightness equally well. If blue and red, for example, with a difference in brightness of 15 degrees were differentiated per-

Table Based on the Tests in Color Perception.

Male.

Colors used	Food in Blue						Food in Yellow									Food in Red														
Red	4	4	3		1	2	3	6	7	1	2	3	2	2	1		6	3	5	7	5	15	12	10	13	14	16	21	27	28
Blue	9	9	12	20	28	24	25	20	13	14	9	6	4	4	3	5	12	7	5	9	7	2	6	3	4	3	8	5	3	2
Dark Gray	12	10	11	7		2		1	3	3	5	6	3			1	3		4	3	3	1	5	3	4	2	2	1		
Light Gray	4	2	3	3	1	2	1	1	3	2	1	2				2	3	2	2	3	6	2		2	2	2		1		
Green		5	1				1	2	1	2	1	3			1	1	2	3	1	2	3	3	3	2		3				
Yellow	1								3	8	12	10	21	24	25	21	4	15	13	6	6	7	4	10	7	6	4	2		

Male. ‖ *Female.*

Colors used	Food in Green												Food in Blue				Food in Yellow				Food in Red			Food in Green					
Red	16	12	12	14	11	7	8	8	4	4	3	2	4	2	1		2	4			6	13	28	25	18	6	3	4	1
Blue	1	2	3	2	2	2	2	1	1		1		8	18	19	26	20	12	5		2	1		1	2	4	1	3	
Dark Gray	4	1			2	1	1						5	5	3	2	1	1			1	2		1			1		1
Light Gray	1	3	1	2	1								5	4	4	2	3	1	1						1				1
Green	5	10	11	7	11	19	18	21	24	25	26	28	4	1	3		1	1	1		1				3	12	19	17	25
Yellow	3	2	3	5	3	1	1		1	1			4				3	11	23	30	20	14	2	3	6	8	6	6	2

The figures in each vertical column indicate the number of times that the animal went directly to each of the glasses within a set of thirty tests.

fectly and two grays differing by 15 degrees, vary imperfectly, then color very probably was the basis of the discrimination in our first series of tests. But if the colors and grays were differentiated about equally well then the question would still be open.

In the first of these control tests apparatus (4) was used. The glasses, numbered from black to lightest gray, 1, 2, 3, 4, 5, 6, 7, 8 and 9, will be referred to by these numbers. These tests were made with the male only. Food was put first into the lightest gray, number 9. It was set down then along with the black, number 1, and the monkey was allowed to take the food. These two glasses were presented together ten times, being exchanged at my back and then set down sometimes by crossing them over and sometimes not. Care was taken to avoid routine and regular position. As far as possible the glasses were put in good diffused light, and in such positions that shadows might not fall upon them in a way to make the lighter seem to be the darker. After feeding in the light gray, in comparison with the black, the black was replaced by the very dark gray, number 2, and 10 more tests were made. In this way we continued down the scale of grays from black to light and back again. To avoid the criticism that the food glass, number 9, may have become soiled or have had some distinguishing mark on it besides the shade, I replaced this glass occasionally and also renewed the paper covering. I could note no difference, however, in the number of correct choices when these changes were made.

Assuming that the first half of these tests (800, or 100 to the pair of glasses) were disturbed somewhat by the mere difficulty of learning the trick, and therefore neglecting these and calculating the per cents of right choices for the last half only we get the following results: with (9 and 1), (9 and 2), (9 and 3), etc., respectively, 100, 99, 98, 98, 98, 95, 87 and 77 per cents of correct choices.[1] When the white (mixed with black to form the grays) differs in two grays by only 48° and 30°, as when 7 and 8 were compared with 9, we see a marked reduction in the right choices. Yet these grays are so very different that the human eye distinguishes them with perfect ease and certainty. The difference of brightness in red and blue, however, was only 15°, but the monkeys distinguished them easily even though they were mixed up with four other colors; whereas here a choice between two grays differing by 30° of white was made with considerable difficulty. If brightness were the basis of the association in the first series of color tests, the red and

[1] The corresponding per cents for the whole 1,600 tests were 97.5, 96, 94, 92, 91.5, 86, 77.5, and 70.5.

blue, differing only by 15 degrees of brightness, should have been confused freely. But they were not so confused. The female, especially, confused these colors but very rarely. We infer, therefore, that the monkeys were probably making their associations on the basis of color and not on that of shade.

This test was followed by a second series of control tests on the following plan, the apparatus being the same as in the preceding tests. I first used glasses 8 and 9 together, putting food in the lighter of the two, in number 9. After 10 tests, 9 was dropped out and 8 and 7 were used, the food being put into number 8, the lighter one. This was continued up the scale of shades and down again, introducing such variations in the order of presentation as would equalize the learning and practice effects.

Taking the results, along with the degrees of white in each of the grays, for the last half of the tests again, it appears plain that in a gross way the per cent. of correct choices agrees with the differences of degrees of white used in forming the grays, and that differences of 27, 32, 35 and 55 degrees only, give results above mere chance; for chance alone should have divided the selections half and half. The difference in the degrees of white between 1 and 2, 2 and 3, etc., respectively, were 35, 55, 27, 18, 15, 17, 18 and 30. The corresponding per cents of correct choices were 94, 83, 68, 52, 46, 56, 50 and 64.[1] These results agree quite well with those of the preceding test, especially if we consider that in the preceding the feeding glass remained of the same shade throughout the whole series of tests. Its image, therefore, may have become somewhat definitely impressed on the monkey. In that event his choices would be based upon an absolute standard in the same way as was suggested in the size tests. In this last series of tests the feeding glass changed with each group of 10 tests. Hence no single image could be of any particular service. It would appear then that if the monkey managed to choose with measurable correctness he very likely had a general notion of a low order, which might be represented by *food-always-in-the-lighter*. The inducement to generalize would be greater in the tests where the change was from the darker toward the lighter grays. For here when 1 and 2 were used the image of 2 might become partly fixed as the feeding glass. But when 2 and 3 were used next the monkey must leave the glass in which he had just been fed and move on from 2 to 3, from a darker to a lighter. That is, shade 2 had to be abandoned and shade 3 selected even though shade 2 was before him. That this was a more severe

[1] The corresponding per cents for the whole 1,600 tests were 86.5, 88, 72, 56, 48, 45; 53.5, and 69.

test than the reverse movement, appears from the fact that here he chose correctly on the average only 69 per cent. of the times, while in the reverse movement, where the lighter glass just used as a feeding glass was each time dropped out and a darker one put in, and where he could, so to speak, lag behind with the lighter glass, he chose correctly on the average 73 per cent. of the times. I do not say that the monkey had generalized, but that we have here something that looks very much like a generalization of a low order. I shall return to the matter in the section on General Notions. The differences of 15, 17 and 18 degrees in brightness with these grays has resulted in utter confusion, while a difference of 15 degrees of brightness between the red and blue resulted in all but a perfect differentiation; hence the inference as in the preceding series is unavoidable, that color was really the basis of discrimination in the color tests.

A third control test was made as follows: apparatus (3) was used to see if 6 grays, varying somewhat more in brightness than the colors did in the color tests, would be selected and associated as readily as the colors were. The shades varied from 215 to 0 degrees of white, while in the color tests the variation was from 215 degrees of white (yellow) to 35 degrees (red). The method of experimenting was the same as that used in making the color tests.

Let us designate the grays from lightest to darkest as A, B, C, D, E and F. Each monkey was fed from the same glass C. It was thought unnecessary to change to other feeding glasses. The male was tested 360 times; the female, 180 times. In the 30 included between the 150th and 180th tests, the male made 13 correct choices; but after that his correct choices fell to as low as 5 out of 30 or to the level of mere chance. In the 360 choices he selected A 27.2 per cent. of the times; B, 18.5; C, 22.5; D, 14.4; E, 8.0; and F, 9.4. Clearly he knew the food was among the lighter glasses, but was unable to distinguish well enough to select accurately. The female in her sixth 30 succeeded in choosing correctly 21 times. She chose A 11.6 per cent. of the 180 times; B, 25.5; C, 43.9; D, 9.4; E, 5.6, and F, 5. In these tests the fact that the monkeys chose B, a glass lighter than the feeding glass, more frequently than D, a darker one, may have been due to a tendency to choose brighter colors and lighter shades, as suggested by Mr. Garner. The male may have been influenced somewhat by his preceding experience in the two series of control tests described above. The female, however, had been fed from gray glasses but 22 times in all, and even then with the food in the darker glass. (See Experiments with Designs.) The results show better association on the part of the female than on the part of the male, but in

neither case does the success compare at all with that attained by each when dealing with the colors. The conclusion that these animals have a clear perception of color and are not dependent upon lights and shades alone, again seems justified.

As a final and crucial test apparatus (2) was used. Shades of gray paper were selected by the "Flicker Method," differing as little as possible from the brightness of the colors used. Glasses were covered with these as before, and a color glass with its three equal gray glasses were used together. They were exchanged on the board and presented in the same way that the colors were in the original color tests. If now the animals do not perceive color they should have been brought into complete confusion with regard to the colors and the equal grays since these necessarily differed very little or none at all in brightness. The three gray glasses were used to give sufficient shifting on the board, and thus to exclude the possibility of choice by mere position.

Sixty tests were made with each color in the order, blue, yellow, red and green, all on the same day. These results are shown in the first of the accompanying tables. The data for the second table were obtained eight days later.

Colors Used.	Male.		Female.	
Blue	17	27	13	26
Yellow	12	23	27	30
Red	26	28	24	30
Green	28	29	30	30

Colors Used.	Male.		Female.	
Yellow	21	27	28	30
Green	27	27	27	28
Blue	26	26	25	30
Red	24	25	26	27

The figures in the tables indicate the number of times that each color was correctly chosen in a set of 30 tests.

In this second series of tests the colors were presented in the order designated in the second of the tables above. Here 30 tests were made with the yellow; then 30 each with the green, blue and red. A second round of 30 each was made immediately afterwards.

The association, with this apparatus, was much more readily formed than in the first color tests when the six glasses were used, partly because the board was now shorter, and partly because there was less of diversity, the three glasses being all of the same gray and there being but one colored glass. The accuracy of the discriminations was so great and the associations so readily formed that there is in my opinion absolutely no question any longer about the monkeys' ability to perceive color.

Color Preferences.

I attempted to study color preferences by feeding the monkeys colored candies, as Mr. Garner did. The candies were handed to the monkeys either on a cardboard or on the palm of my hand. They were separated some distance, and the relative position of the colors was frequently changed. The card or hand was always withdrawn as soon as one of the candies was taken. The male was so much frightened by my coming near the cage that, although he sometimes took the candies, he could hardly be said to have chosen between them. After feeding the female about 100 times she resorted to grabbing with both hands, apparently to make sure that neither of the pieces of the candy should escape her. After the female adopted this method no further tests of this kind could be made with her. No conclusions can be drawn from these tests of mine with reference to color preference. This method of testing in my judgment is very unsatisfactory. A little variation of the card or the hand from right to left may determine the selection, since the distance that the animal moves in getting to them is so small. Further, with a timid animal one gets nothing deliberate enough to partake of the nature of a choice. Most of the animal's attention is given to the experimenter, and it simply makes a quick grab for that piece of candy which is in easiest reach from its position.

Another attempt was made, using a method similar to that employed in making the tests on color perception. I covered the cylindrical surfaces of glasses with the following colors of paper: green, blue, orange, dark gray, red, yellow, violet and white. As to their brightness I will here say only that the blue, red, green and yellow were the same as used in the preceding tests. The orange was slightly darker than the yellow, and the violet was darker than the blue.

Experiments were made with the male first. I presented the four glasses, blue, green, orange and dark gray together. A morsel of food was put into each glass, and the animal was allowed to take all the food before the glasses were taken up. The order of the glasses was changed as before. After 30 presenta-

tions the other four glasses were used. In the first group the first choices were: green 12, blue 1, orange 11, and dark gray 6; in the second: red 2, yellow 12, violet 5, and white 6. Two new groups were now made, each consisting of the two colors most frequently chosen in one group and of the two least frequently chosen in the other group. Presenting the glasses as above, the choices for the first group were: green 5, orange 10, violet 5, and white 10; for the second group dark gray 4, blue 4, yellow 13, and red 9. As a total result so far we have yellow 25, orange 21, green 17, red 16, white 16, dark gray 10, violet 10, and blue 5. Again I threw the glasses into two groups, those most frequently chosen and those least frequently chosen. Now the choices for the first group were: yellow 11, orange 9, green 9, red 1; for second: dark gray 6, violet 7, white 10, and blue 7. The second group here, however, has no value as after about 8 choices he fell to starting in with the glass on the right and to taking all as they came.

The female from the very first began at the left of the row and took the food from each glass in the order of its position. If she has any color preference whatever the association of order completely covered it up. Still hoping to get some signs of color preference from her by this method I selected the two colors which had appealed to the male most and least—yellow and blue. In 30 presentations of these, the food in both glasses, and alternating them right and left, she chose the left glass 28 times. Evidently this indicates a continuation of the former habit. In the other two cases a little noise made by the male in his cage veered her over to the other glass. In experiments of this type, color preference would probably be based on either an æsthetic taste or on the fact that certain colors lie nearer the animal's feeding instincts than others do.

One more series of tests was undertaken in the study of color preference. A cage three feet long, ten inches wide and fourteen inches high was so constructed that the back part could be moved as a sliding door. The inside was painted a light gray. This gave the inside of the box a uniform appearance with no attractive points for gnawing or escape. Eight inches in front were hung two large sheets of colored gelatine—blue and orange. The arrangement was such that very little light could enter the cage other than through these sheets. A vertical partition opposite the middle of the front of the cage and between it and the sheets of gelatine, greatly reduced the animal's opportunity, when in one end of the cage and behind one of the colored sheets, to see and be influenced by the other color. The cage was so placed that direct sunlight flooded the whole of it. One animal was turned into the cage at a time and kept there for twenty minutes. After ten minutes the places of the col-

ored sheets were exchanged. For observation I sat some distance in front of the middle of the cage. By means of the cumulative stop-watch the total time that the monkey spent behind each color could easily be kept. The assumption was that if either color was more pleasing, or less painful to the monkey than the other he would make choice between them and sit in the color most agreeable. The female was in the box during 14 ten-minute periods. In 5 of these she spent more time behind the blue than behind the orange, and 56 per cent. of her total time was spent behind the orange. The male, likewise, was in the cage 14 times, 9 of which were taken up mostly behind the blue. He spent 62 per cent. of his whole time behind the blue. The results were regarded as so very irregular as to indicate neither preference nor the absence of it, and further experimenting by this method was abandoned.

The first time the monkeys were turned into this cage they seemed to be surprised at the change of color on their own bodies; for several times they looked strangely up and down and along their arms, and tried to pick the color off of their hands. This apparatus, in addition to appealing to an animal's æsthetic taste and instinctive habits of feeding, might appeal, also, to his instinctive fear. A timid animal might stay where he considered himself best concealed, and a bold one where he could get the most satisfactory view of his enemies and environment.

The results of these tests on color perception and color preference may be summed up as follows:

1. There can be no doubt that monkeys perceive colors.
2. Two grays having a given degree of difference in brightness are not discriminated as well as two colors having an equal difference in brightness.
3. For accurate discrimination of difference in brightness a difference of about 35 degrees or 9 per cent. of the white constituent of the gray is necessary.
4. The monkeys are able to distinguish colors from grays though the brightnesses are the same.
5. The male appears to have a preference for bright colors, but blue seems to be discriminated against.
6. In two instances there were indications of at least a low form of general notion.